

Edward F. Bryant and Laurie L. Patton, eds. The Indo-Aryan Controversy: Evidence and Inference in Indian History. London and New York: Routledge, 2005. Pp. xi + 522.

"Teach the controversy!" Such is the rallying cry of the proponents of Intelligent Design, or ID for short, (formerly known as Creation Science, formerly known as Creationism) and (if I may mix my metaphors) the Trojan Horse with which they smuggle this religious doctrine into the teaching of science in our public schools. It is of course difficult for those of us who believe in liberal education and the free exchange of ideas to oppose such a sentiment, or the also apparently laudable notion that school children should be exposed to both sides of the issue, a paraphrase of George W. Bush's recent statement about ID. But the problem is, there is no controversy, at least no scientific one; the two sides are rather the scientific consensus and the religious opposition to it. Intelligent Design further seeks to exploit disagreements in detail among evolutionists to throw doubt on the theory of evolution in general, and to pervert the meaning of the scientific term "theory" to suggest that evolution is a vague and unprovable hunch about life on earth.

I could not help thinking constantly about this case throughout my reading of the book under review, for the parallels between the Intelligent Design issue and the Indo-Aryan "controversy" are distressingly close. The Indo-Aryan controversy is a manufactured one with a non-scholarly agenda, and the tactics of its manufacturers are very close to those of the ID proponents mentioned above. However unwittingly and however high their aims, the two editors have sought to put a gloss of intellectual legitimacy, with a sense that real

scientific questions are being debated, on what is essentially a religio-nationalistic attack on a scholarly consensus. In such cases it is tempting to ignore the whole thing, especially the acrimonious words and ad hominem attacks (made by both sides) of those who choose to engage, on the assumption that it is a sideshow to real scholarship. However, I have reluctantly agreed to review the book because the question is properly a linguistic one, and linguistic arguments are often hard to follow even for a scholarly but non-linguistic audience. Neither of the editors is a linguist by training, and they therefore have been credulous about accepting patently weak or false linguistic arguments, and this in turn has led them to be more even-handed in their assessment of the material presented than it merits.

The volume consists of thirteen contributions by various scholars, more or less evenly divided between pro's (i.e., upholders of the scholarly consensus) and anti's (i.e., opponents of it), with a lengthy introduction (1-18) by one editor (Patton) and even lengthier "Concluding Remarks" by the other (Bryant). The articles are divided into four sections, "Archaeology" (19-104), "Archaeology and Linguistics" (105-177), "Philology and Linguistics" (179-404), and Historiography (405-506, which includes Bryant's concluding remarks [468-506]).

Before going further I should perhaps briefly sketch what the "controversy" is. The nub of the matter is the nature of the relationship between the Indo-Aryan languages of the subcontinent, i.e., Sanskrit and its descendants first attested in historical times in the northern parts of the subcontinent [as opposed to the Dravidian and Munda languages primarily attested in the southern and middle parts] and the (other) Indo-European languages, found from Central Asia and the Near East through most of Europe. As I hardly need to

remind readers of this journal, it has been generally accepted for nearly 200 years that all these languages are genetically related (a linguistic, not a biological term, NB) and can be traced back to one original language, Proto-Indo-European, for which we have no attested remains but which can be reconstructed in remarkable detail. Both the demonstration of linguistic relationship and the reconstruction of the proto-language rely on a very powerful and scientifically exact tool known as the Comparative Method, whose value has been demonstrated time and time again on language families around the globe. There is nothing either vague or speculative in the application of these linguistic methods.

If the relationship among all these languages is accepted, then the next questions concern how they became so very different and how they came to occupy the geographical positions that they do when they first enter history. It is the latter question that sparked the "Indo-Aryan controversy": how is it that the Indo-Aryan languages are found in the South Asian subcontinent, as opposed to all their sister languages (including their closest relatives, the Iranian languages), which are widely distributed outside it? The standard answer to this question has long been that Indo-Aryan entered India from outside, coming first into the extreme northwest, where, in fact, we find Vedic Sanskrit, the earliest of our IA languages. Only later did the Indo-Aryan languages move south and east through the territory. Challenges to this long-accepted answer produced the Indo-Aryan controversy, and these challenges can take one of two basic shapes: to deny the linguistic relationship entirely or to assert that Indo-Aryan holds the geographical position it always has, and it is the other Indo-European languages that moved, out of India, to expand into the vast areas where we first find them.

(A variant of this latter view is that Sanskrit is not the daughter of an unattested proto-language, but the attested mother of all Indo-European languages.) Most of those in this volume who are opposed to the standard theory support some version of the "out-of-India" theory.

It might seem at first that when we come to questions of geographical position, of how languages came to be where they are, we enter the domain of archaeology, but in fact it is critical to remember that this is still the domain of linguistics, with archaeology only tangentially relevant. For we must not confuse movements of languages with movements of peoples. Languages can spread to new territories in a number of ways, only one of which is through the migration (or "invasion") of people who speak the language. Moreover, even movements of people can leave little trace on material remains -- because they adopted the indigenous material culture or because their small numbers relative to the indigenous population kept them from leaving archaeologically observable remains. So most of the means of linguistic movement are invisible to archaeology; indeed, in the absence of written documents, language is invisible to archaeology: different archaeological complexes can speak the same language, and similar archaeological complexes different ones. (The archaeologist C. C. Lamberg-Karlovsky writing in this volume repeatedly issues similar cautions about making correlations between archaeological evidence and language.) Therefore, though archaeology might offer useful ancillary evidence with regard to the Indo-Aryan question, linguistics must offer the primary evidence, and scenarios that are highly implausible or impossible from a linguistic point of view are unlikely to be correct.

It is therefore disturbing and misleading that the editors of this volume choose to devote the first part of it to archaeology, not linguistics -- thus allowing the justifiable guardedness of J. M. Kenoyer and the more virulent anti-linguistic bias of J. G. Schaeffer and D. A. Lichtenstein in Part I to set the terms of debate. (B. B. Lal's article in this first section is hostile, amateurish, and out-dated, and does his cause no good.) The second part, Archaeology and Linguistics, has little direct bearing on the question (and fairly little to do with linguistics), though, as noted above, Lamberg-Karlovsky's sensible remarks about identifying the linguistic affiliations of archaeological complexes are important to keep in mind.

The debate must then be decided (if decided it can -- and if it is worth dignifying this muddle with the word "debate") by the third section, Philology and Linguistics, and if this is so, then the into-India language-migration theory must be declared the victor, if only by default. The two spokesmen for the contrary side who attempt to grapple with linguistic data, S. S. Misra and K. Elst, simply fail dismally. (Two other contributions by out-of-India proponents, S. Kak and S. G. Talageri, have nothing to do with linguistics, despite being included in this section.)

The editors did no favor to the reputation of the late Prof. Misra by including this posthumous article in their volume, for it is painfully naive, amateurish -- indeed, I'm afraid I must say, incompetent. He seems to have no understanding of the principles and methodology of scientific historical linguistics and little exposure to any Indo-European scholarship after Brugmann (in the late 19th c.) save for his own. He has a tendency to make breathtakingly apodictic statements for which he either offers neither evidence nor argument, or such muddleheaded justification that it roused me to pity: e.g., that Indo-

European grammar is 100% Sanskrit grammar (p. 184), that the Law of Palatals, a cornerstone of Indo-European linguistics whose validity is overwhelmingly demonstrable across the family, is false (pp. 184ff.), that the Rig Veda can be dated to 5000 BC (p. 187), that Dravidian is Indo-Aryan (p. 191), that the laryngeal theory is wrong (p. 212). He cannot even cite Sanskrit forms correctly (see, e.g., the non-existent and phonotactically impossible *hand* [for *hánti*, p. 184]). I could continue, but I will be accused of condescension and ridicule (see below). However, if Misra was chosen to be the linguistic standard bearer for the anti-forces, as his initial position in the section suggests, it is a sad commentary on those forces -- that no one with minimal competence in the field of Indo-European linguistics could be found to argue the case.

Elst's article is more sophisticated and responsible than Misra's, but the fact that he is not a linguist and works entirely from secondary materials flaws his contribution: he seems not really to understand linguistic concepts and argumentation (e.g., loan word phonology [p. 250], derivation vs. "artificial coinage" [p. 262], language "mixture" [p. 272]) and so characterizes them rather crudely, and then in turn either dismisses them prematurely or builds arguments upon them that they will not support. Elst's specialty is offering alternative explanations for the numerous strong arguments that have been put forth for the into-India hypothesis -- e.g., how to explain the characteristic and complex patterns of isoglosses and subgrouping among the other Indo-European languages if they all came spurting out of NW India like toothpaste out of a tube (pp. 243ff.). He himself characterizes one of his own alternative hypotheses as "counter-intuitive but not strictly impossible" (p. 274), a phrase that could be applied to all of his hypotheses. And since his alternative model, the out-of-India

model, depends on all of these implausible hypotheses being true at once, it is time to invoke Occam's razor.

Of the remaining two contributions to Part III, H. H. Hock carefully and sensibly demolishes arguments (from both sides) that are based on dubious interpretations of Vedic passages (e.g., one on which Kak in the immediately following chapter bases his entire Vedic chronology [p. 325]), but does not comment directly on the question the volume addresses. It thus remains to M. Witzel to represent the other side, a role to which he is accustomed. His contribution is, as usual, lengthy and aggressive and sometimes goes too far in sketching a detailed (and indeed often plausible) history of the migrations into India, based in part on linguistic paleontology. But his remarks throughout on the misuse of data and the misunderstanding of linguistic methodology (see, e.g., p. 350: "the linguistic ideas and 'arguments' of the autochthonists are far off the internationally accepted norms and procedures") seem, sadly, to need constant repetition, as some of the contributions just discussed make clear.

I now am required to admit that much of the tenor of my review, if not its exact wording, has been anticipated by the editors. Patton, for example, in her Introduction begins by saying that she expects that each side will "find us overly accepting and affirming of the other side," a fence-sitting strategy that she considers "an intellectual virtue" (both quotes p. 1) Bryant in his Concluding Remarks repeatedly implies that taking a firm position on the issue is a rejection of "objective scholarship" (see, e.g., p. 500), though his statements elsewhere that "much of the evidence ... can prove to be extremely malleable" (475) or "amenable to renegotiation" (484) shed new light on the term objective scholarship. Such

expressions put the reviewer in an untenable position: if I do my job as a reviewer and point to serious flaws in methodology, evidence, and argument, then I will be guilty of "a mocking and condescending tone" and "simply highlighting and ridiculing the most outlandish aspects of an opponents [sic] argument," in disputing the value of giving "a fair and adequate representation of the differences of opinion on the matter" (all three quotes p. 500). But in regard to "differences of opinion" it is quite often the case that one opinion is simply wrong, and I will end by saying that scholarship is not journalism, where lack of bias is often enacted by allowing two incompatible sides "equal time" with no attempt at adjudication. I believe that a commitment to objective scholarship requires us to take a definite stand when the evidence on one side seems to be overwhelming and the arguments incontrovertible, and that appears to me to be the case here.

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